I am commenting on this manuscript because I think the equations presented and analysis methods utilized in this work require greater explanation and clarification. I believe there are a combination of typographical errors (potentiall) and conceptual issues to address in the analysis.

1. First, it seems the authors equate the mobility diameter inferred from DMA measurements without correcting for the size of the gas molecule (or if they are correcting for gas molecule physical size, they are not stating this). As per Larriba et al (2011, doi: 10.1080/02786826.2010.546820), the physical diameter of a particle in air is ~0.3 nm smaller than the mobility diameter. If using mobility diameter for collision rate calculations, this lack of correction could have a huge consequence at smaller sizes (e.g. (2.8 nm/2.5 nm)^2 = 1.25, a 25% difference in hard sphere predicted growth rate). This seems that it would ultimately effect the collision rate calculated, and interpretation of the Hamaker constant.
2. Equation S1 has a factor of 2 in the numerator that is either not correct or not fully justified. The authors state that "$k_{coll}$is the kinetic collision frequency between particle and vapor, which is accounted for twice to include collisions in both ways." What does "both ways" mean? Why would something grow at twice the condensation rate?
3. I think equation S2 is not correctly written, or at least, it has the wrong limits. When the ratio k_D/k_K(Knudsen number for collisions) is large, then it does correctly converge to k_K. However, it appears that as this ratio becomes very small, equation S2 would approach zero, when it needs to converge exactly to k_D to be an accurate transition regime kernel. In Chan and Mozurkewich (equations 27 and 28 of their paper), the do provide different expressions than what is given here. However, these equations would also have the same issue, (an incorrect limit). The authors should look at the transition regime expressions of Fuchs or Dahneke (or more recent developments) to see how the transition regime equation must be constructed to converge to the correct limits.
4. The authors appear to be using equations from Chan and Mozurkewich which have typographical errors in the original manuscript and are copied here. Just below equation (S2), the author note that the kinetic regime rate coefficient k_K is given by the expression:

$$k_K = \frac{\pi}{8}\left(d_p + d_v\right)^2 \sqrt{\frac{8kT}{\pi m_{p,v}}}\, E(\infty)$$

However, k_K should be the product of the projected area of the combined particle-vapor molecule at contact, their relative mean thermal speed, and an enhancement factor (Chan and Mozurkewich have the same typographical error). Therefore it should be:

$$k_K = \frac{\pi}{4}\left(d_p + d_v\right)^2 \sqrt{\frac{8kT}{\pi m_{p,v}}}\, E(\infty)$$

Like the gas molecule correction, this change will affect the inferred Hamaker constant. At the same time, oddly enough, as written, the authors' extra factor of 2.0 and their extra division by

2.0 would cancel out in the free molecular regime. However, then their equation for k_D would not be correct with the factor of 2.0 in equation S1 included.

5. Aside from the issues with equation S2, use of the equations of Chan and Mozurkewich alone, which come from the theoretical derivations of Sceats, may not be accurate. More recent theoretical investigations of van der Waals enhancement in collision rates (doi: 10.1063/1.4742064) suggest that for a given Hamaker constant, Sceats overestimates the enhancement factor in the free molecular limit. The noted reference discusses more tractable approaches which agree with collision rates from trajectory calculations.
6. Equations S4 for E(0). I think the authors should show that this functional form follows exactly from the Fuchs integral for the enhancement factor in the continuum/diffusive regime, which is an exact integral and typically easily calculable for a given potential interaction. Similarly, it is not clear that equation S3 follows from analyzing ballistic regime in the appropriate manner. The authors should be aware that the equations in Chan and Mozurkewich do not appear in the original Sceats paper, as far as I can tell they are a regression fit to results in Sceats's work (from his plots).
7. It would seem more reasonable for the authors to use a different approach either in lieu of or in addition to the equations of Chan and Mozurkewish, i.e. for the authors to either compare to the equations of Ouyang et al (doi: 10.1063/1.4742064) and/or of Fuchs & Sutugin (doi: simple but accurate: 10.1016/0095-8522(65)90031-0). In the free molecular limit, Ouyang et al follow Fuchs's approach but integrate across the velocity distribution, while Fuchs assumes a single approach speed.